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SPECULATIVE SCIENCE.

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"*Wenn ein Kopf und ein Buch zusammenstossen, und es klingt hohl, muss es denn immer das Buch gewesen sein?*"—LICHTENBERG, *the Physicist.*

THE above title is prefixed to an article contributed by Professor Simon Newcomb to the April number of the "International Review." The avowed object of that article is to discredit a recent volume of the "International Scientific Series" ("The Concepts and Theories of Modern Physics") as a publication unworthy of the company in which it appears, and to denounce its author as a person ignorant of the subject whereon he writes—as a scientific, or rather unscientific, "charlatan" and "pretender" belonging to the class of "paradoxers" whom Professor De Morgan has immortalized in his famous "Budget." I am fully aware that, as a rule, it is both unwise and in questionable taste for an author to make direct reply to criticism, however hostile, baseless, or absurd. The merits of a book must find their vindication, at last, in its contents, and the chief function of the critic is to bring them to the attention of the reader, the value and spirit of the critical performance being of secondary importance. But the case in hand appears to me to be an exceptional one. The unmistakable intent of Professor Newcomb's "criticism" (and, if it be left unchallenged, its probable effect) is to signalize the contents of the book with which he deals as mere drivel, and unworthy of a moment's serious attention. And he writes for a magazine, the majority of whose readers, however intelligent they may be, can hardly be expected to possess that familiarity with the matters under discussion which is a necessary prerequisite to the formation of an independent and trustworthy judgment. All they are likely to know and care is,

that Professor Newcomb is a prominent scientist, at the head of a scientific bureau in Washington; while the author of the book he professes to review, if known at all, is known only in connection with pursuits which are generally supposed to preclude, not only distinction but even reputable standing in the domains of scientific investigation. I take the liberty, therefore, to subject the strictures of my critic to a counter-critical examination, trusting that the learned professor himself will find it thorough, and that the reader who has not only perused his article, but also looked into a chapter or two of my book, will recognize it as neither impertinent nor unfair.

Whatever may be thought of the soundness or unsoundness of the general argument of the little book in question, the drift of that argument, it seems to me, can hardly be mistaken by the reasonably intelligent reader. What I attempt to show is simply this : that modern physical science aims at a mechanical interpretation of physical phenomena, seeking to effect a reduction of them to two elements which are ordinarily designated as *matter* and *motion*, but which (for reasons briefly stated in the book, but to be stated more at length presently) are more correctly designated as *mass* and *motion*. I then attempt to show that, if to these premises we add the assumption of the atomic constitution of matter, the mechanical theory necessarily involves four distinct propositions, relating severally to the equality, inertia, and inelasticity of the atoms or ultimate molecules and the essentially kinetic character of what is now universally termed energy. In order to enforce the irrecusability of these propositions on the basis of the atomo-mechanical theory, and to guard against the imputation that I am engaged in the frivolous pastime of chopping logic, I am at pains to show, in the next four chapters, that every one of these propositions is insisted on and propounded in terms identical with, or equivalent to, those in which I state them, by men whom I was under the delusion, up to the time of the appearance of Professor Newcomb's article, of regarding as persons of the highest scientific authority—such men as Professors Du Bois-Reymond, Thomas Graham, Wundt, etc. I then proceed to inquire what is the relation of these propositions to the sciences of chemistry, physics, and astronomy, as they are actually constituted, endeavoring to ascertain whether or not the fundamental propositions of the atomo-mechanical theory are available as theoretical solvents of the facts with which these sciences are conversant, and whether or not they are consistent with them. The result of this inquiry is, that the man of science, however emphatic he may be in the general assertion that all physical phenomena are due to the interaction of atoms or ultimate molecules, is constrained by the data of scientific experience to repudiate and discard the propositions which his assertion necessarily involves. It thus appears that there is conflict between the facts and working hypotheses or theories of the sciences on the one hand and the atomo-mechanical theory on the

other; that the latter theory fails in the presence of the facts, and that all attempts to remove this conflict have, thus far at least, been abortive.

After supplementing these preliminaries by a discussion of the atomic theory and its dependant, the kinetic theory of gases, I approach the problem whose solution is the sole aim of my little treatise, which, as is expressly stated in the very first sentence of the preface, is designed as a contribution, not to physics or metaphysics, but to the theory of cognition. That problem is the determination of the logical and psychological origin of the mechanical theory, and of its attitude toward the laws of thought and the forms and conditions of its evolution. It is neither necessary nor practicable here to attempt a reproduction of the tenor of my discussion. It is sufficient for my present purpose to state my conclusion, which is, that the mechanical theory with all its implications is founded on a total disregard or misapprehension of the true relation of thoughts to things or of concepts to physical realities; that, so far from being a departure from and standing in antagonism to metaphysical speculation, the propositions which lie at its base are simply exemplifications of the fallacies that vitiate all metaphysical or ontological reasoning properly so called. There is hardly a page in the book, after the first two expository chapters, in which my utter repudiation of the mechanical theory and its fundamental assumptions is not conspicuous. My objections to this theory are stated in so many ways, and are enforced by so many considerations, that my position in regard to it appears to me insusceptible of misapprehension even by the most hebetated intellect. During the last six weeks I have received more than twenty letters from various persons—most of them mathematicians and physicists, but a few of them persons without scientific training—in which the doctrines of my book are discussed or questioned, sometimes on grounds which indicate that my meaning has been strangely misapprehended. But not one of these letters gives rise to the least suspicion that the writer was mistaken as to my attitude toward the mechanical theory.

And now, what does Professor Newcomb represent my position to be? The reader who has not seen his article will be amazed when I tell him that, according to him, my book was written for the purpose of *maintaining* the propositions of the atomo-mechanical theory, and of subverting the whole science of physics by means of them, on the principle, I suppose, that if the facts do not agree with the theory, so much the worse for the facts! Here is Professor Newcomb's language:

> The author's criticism is wholly destructive; where he constructs it is only to destroy. It is true that his first chapter on the atomo-mechanical theory lays down certain propositions already mentioned which he seems to hold as true. He makes use of them to destroy the whole fabric of modern physics, and show physical investigators generally to be the subjects of miserable delusions. But

his last chapter is devoted to showing that this theory is itself a failure, so that, when he takes his leave, we have nothing left to contemplate but a mass of ruins.

It is curious to note the introduction of the word "seems" into this passage—as the lawyers say, its appearance with a *semble*—while in other places, e. g., where Professor Newcomb speaks of the proposition that molecules are inelastic as my "favorite doctrine," or where he charges me (after reading my tenth chapter!) with ignorantly confounding the "abstract noun" *mass* with the concrete term *matter*, he makes no such qualification.

Having satisfied himself (no doubt before writing his article, though the conclusion is stated most explicitly toward its close) that I am in the lists as a champion of the atomo-mechanical theory and as the dogmatic defender of its fundamental propositions, he proceeds to assail these propositions, sometimes with what he seems to regard as an argument, but generally with a sneer. The contents of my introductory chapter, consisting almost exclusively of citations from the writings of Professors Kirchhoff, Helmholtz, Clerk Maxwell, Ludwig, Du Bois-Reymond, etc., he brands as "propositions in which we can trace neither coherence nor sense." The thesis that, on the basis of the atomo-mechanical theory, all potential energy is in reality kinetic—the distinct proposition of Professor P. G. Tait, who asserts it as the unavoidable consequence of the atomo-mechanical theory of gravitation—he "passes over as not even worth quoting." Similarly the doctrine of the essential passivity of matter—also a proposition of Professor Tait, whose exact words I quote on page 306 of my book—is flouted with the disdainful remark that "such words as 'active' and 'passive' have no application in the case and serve no purpose, except to produce confusion in the mind of the reader." In this way he levels his thrusts at the most eminent physicists and mathematicians of the day, laboring always under the hallucination that he is striking at me.

Among the most characteristic performances of Professor Newcomb are his strictures, already adverted to, on my substitution of the term *mass* for the word *matter*, in designation of the substratum of motion in the light of the atomo-mechanical theory. According to him, this use of the word *mass* is evidence of my ignorance and intellectual confusion, as well as of my "total misconception of the ideas and methods of modern science." He informs me that the word *mass* is "an abstract noun like *length*," whereas I use it "as a concrete term, and in *nearly* the same sense as we commonly use the word matter." And thereupon he delivers himself of a dissertation (which resembles nothing so much as a sermon of "Fray Gerundio" to his "familiars") on the necessity of using scientific terms only in accordance with their exact definitions, of ascertaining the meanings of the words *mass* and *motion* by a reference to the methods whereby they are measured, and

so on. All this is certainly strange news to an author who has devoted several chapters of his book to the task of showing that the great fundamental vice of the mechanical theory is the confusion of concepts with things, and particularly of the connotations of the concept *mass* with the complement of the properties of *matter*—who, in a word, is guilty of the great offense of expressing, in the precise terms of the science of logic, what Professor Newcomb is staggering at with a phrase borrowed from some elementary treatise on grammar!

And here I am tempted to do a little Gerundian preaching myself, Professor Newcomb being, of course, my congregation of "familiars." Here is my sermon: *Hombre sabio y admirado*, scattering supernal wisdom, like hurling thunder-bolts, is a prerogative of the dwellers on Olympus, not to be usurped by a drag-footed philosopher bellowing at its base. *Quod licet Jovi, non licet bovi.* I do not mean to question your general ruminant powers; but you have delivered yourself of some things "that have not been well digested," and had better be chewed again. Let me see how I can help you. Listen: When we speak of matter, we mean something which not only has weight, proportional to its mass, but which has all manner of properties—optic, thermic, electric, magnetic, chemical, and so on. Now, in the light of modern science, all these "properties" are regarded as *modes of motion*, if I may be permitted to use the expression of Professor Tyndall. And when we strip matter (in thought, you understand) of all these modes of motion, we have nothing left but *inertia*, which is but another name for mass. This mass is not a concrete thing, but a concept or a part of a concept; it is, as you say, "an abstract noun like length." And the trouble with the atomo-mechanical theorists is their fancy that this abstraction is a thing in itself, something you could look at if you had a telescope with sufficient magnifying power, or which you could weigh and measure if you had a pair of scales or a chemical reagent sufficiently delicate. They labor, as you see, under a huge mistake, which, in charity, ought to be corrected. Whenever you find real matter, you have *mass* and the modes of motion in indissoluble synthesis and conjunction. But when this synthesis is broken by the destructive analysis of the mechanical theorist who persists in saying that things consist of *matter* and motion, you are bound to tell him that what he calls *matter* is not matter at all, but only something which, by a curious law of our thought, we are bound to conceive or imagine as a substratum of motion—the word *substratum* being a barbarous Latin term which in a rough way signifies what is supposed to underlie motion. The term *matter*, as used by those deluded people who think that all the facts of this world can be explained by a resolution of them into matter and energy, or matter and motion, denotes simply what the physicist who knows what he is talking about calls *mass*.

And now, mind, what I have just told you is not some shallow con-

ceit hatched under my own time-tonsured pate, but genuine wisdom which I have simply borrowed from an old, clear-headed fellow, who lived and died a long while ago—Leonhard Euler. If you will read his seventy-fourth letter to a German princess, written on the 11th day of November, 1760, you will find it all set forth at great length. In reading it you must bear in mind, though, that in Euler's time the imponderables, as they were then called, were not so distinctly known or believed to be modes of motion as they are now. And you must also remember that he was writing to a princess who probably knew more about madrigals and operatic airs than about scientific terms, in consequence whereof his exposition became a little diffuse. If, however, you should reject old Euler's reasoning as "belonging to a past age of thought," which, I see, is one of your favorite ways of getting rid of irrefutable truths, I may refer you to a gentleman who is yet among the living—Hermann Helmholtz. You will find what he has to say on the "matter in hand," on the third and fourth pages of his first essay, "Ueber die Erhaltung der Kraft" (not included in the collection of his essays).

Now, *hombre querido* (I am still preaching), if after this you will carefully read again the first twelve chapters of my book, you will probably find that they are somewhat less absurd than you fancied they were. But you will say, no doubt—in fact, you *do* say, though not in so many words—that all this is mere speculative trash, in which the man of science has no concern. One of my reviewers in the New York "Critic"—whom I at one time suspected, perhaps unjustly, from certain peculiarities of his phraseology, and from the fact that, like yourself, he sneers at me for having "wasted" two long chapters on transcendental geometry, of having had oral confabulations with you, in which the mouth of the speaker was not and could not be applied to the ear of the listener—disposes of my discussion of the relation of the mechanical theory to the laws of thought by the following oracular dictum (a travesty of a saying of Carlyle): "A sound digestion has little self-consciousness of the operations of the stomach; the sound thinker gives himself little uneasiness respecting the laws of thought." I can not stop, at this moment, to show you how and why a little knowledge of the laws of thought is useful to the physicist and mathematician. I shall come to that by-and-by, when I have considered what you say about the kinetic theory of gases and space of an indefinite number of dimensions. For the present I only want to tell you how I ventured upon the audacity of intruding the theory of cognition into the science of physics.

In Europe, as well as in this country, there are certain idle fellows who, during the first half of the present century, for want of more useful occupation, took to tracing the ramifications of forms of speech, and finally got to digging for their roots. These absurd persons abound chiefly in Germany, where, as you know, the people are always

in the nebular regions, when they ought to be fighting and grubbing on the solid ground below. In course of time these individuals, despite the utter fatuity of their undertaking, persuaded themselves that they were engaged in something important, and became noisy and presumptuous. At one time they even clamored for admission into the ranks of the physicists and astronomers, on the ground that they had discovered phonetic and other laws, which they claimed to be as immutable as the laws of Kepler. Their application was, of course, scornfully denied, for the reason that they were either no scientists at all, or at best speculative scientists. Instead of submitting humbly to this just decree of the physicists (it is a pity they had not my present meekness before them as an example), these men grew wrathy and turned away with something like this objurgation : " Well, never mind, the time is not far distant when you will come as suppliants to us." And, thereupon, in sheer malice, having got well-nigh through with the roots and branches of words, they fell to attacking the history of their meanings—of concepts, as they called them—pretending to make legitimate employment of inductive methods, which they wholly misapprehended, no doubt, and which, at any rate, were among the clear prerogatives of the physicists. And now they pretend to have established, *inductively*, a number of laws relating to the operations of the intellect, which they again assert to be immutable, and, though controlling acts of consciousness, to be wholly independent of deliberate intent or set purpose. They say, for instance, that there runs throughout the history of speculative as well as of ordinary thinking an almost irrepressible tendency to hypostasize concepts, or (as I have called it, cribbing an outrageous barbarism from Professor Bain) to *reify* them. I will try to explain to you what that means, as nearly as possible in your own words. When people make or find a new "abstract noun," they instantly try to put it on a shelf or into a box, as though it were a thing ; thus they *reify* it. In very early times they did worse than that—they undertook to incase it in a smock-frock or a pair of breeches. They *personified* it. There was a still earlier period when, worst of all, men blasphemously and impiously *deified* abstractions ; and it is said that this class of persons has not wholly died out yet.

Now, the silly speculators I have just alluded to have already divided the science they pretend to be cultivating into several branches, to which, being word-mongers, they give all sorts of sesquipedalian names, such as comparative linguistics, comparative psychology, comparative mythology, and so forth. To give you an idea of the temerity of these pseudo-scientists, let me tell you that one of them, Professor Max Müller, of Oxford—who is, of course, a German—at one time undertook to account for the monotheism of the Jewish race by a peculiarity of Semitic speech. It is even whispered that he and others, years ago, evolved the whole city of Troy, with all its houses and

walls, the heroes within it, with their wives and children, as well as the Greek warriors and their ships, without it—everything, including the Trojan horse and what it contained—from a parcel of solar myths, demonstrating to their own satisfaction that all these persons and things were, at bottom, nothing more than "objectivations" of forms and laws of speech. As was to be expected, this fine theory came to grief when Schliemann appeared with a pickaxe and spade. As usual, the theory collapsed in the presence of the facts. Be that as it may, there is one thing these scientific pretenders persist in asserting, in spite of all their past discomfitures: that more than three fourths of the controversies in theology and metaphysics have had their rise in the ignorance of the fathers of the Church, and of mediæval and modern scholastics, of the results brought to light in these new-fangled sciences. Unfortunately, when I was less old and wary than I am now, I fell in with these "paradoxers," some of whom I knew to be men of great learning, and believed to be persons of thorough earnestness of purpose. To my astonishment I found two mathematicians among them—Hermann Grassmann and Franz Woepcke. I had read with some difficulty, but, as I thought, with reasonable grasp of his meaning, the "Ausdehnungslehre" (since supplemented by a new treatise under nearly the same title, and a number of articles in Crelle's and Borchardt's "Journal") of Grassmann; and I had attempted to read some of the writings of Woepcke, though without success, because he went far beyond my depth. But I got an impression that both had things to say—in mathematics, at least—that were worth knowing; and inferred that there must be sense and purpose also in their linguistic endeavors. In this way I became interested, and gradually caught the spirit of the comparative linguists and mythologists by contagion. And so it came to pass that, after a while, I asked myself this question: "If the results of these sciences are available for the solution of the perplexities of the metaphysicians, why may they not also throw some light on the nature of our perplexities in physics?" So far as I could learn, no one had attempted an orderly and systematic answer to this question, although (as is not unusual in cases of this sort) there was a considerable amount of scattered material ready to the hand of whomsoever should undertake the work. Under these circumstances, I was fool-hardy enough to make an attempt myself, the result being my poor little book. And now I confess I am not a little mortified at being informed that I am a "learned and able" idiot; and I derive but scant comfort from the assurance that my mental predicament may be accounted for on the theory of contagion, and that the hypothesis of congenital imbecility may be avoided.

But it is time to doff my Gerundian robes and to cease apostrophizing the familiars, for I have things to say which ought to be said in all earnestness and sobriety. I am about to examine Professor

Newcomb's animadversions on my chapters on the kinetic theory of gases and transcendental geometry. On the former he expatiates as follows :

> For the benefit of the non-scientific reader we may say that there is no theory of modern physics, the processes supposed by which are invisible to direct vision, which is more thoroughly established than this. It explains with the utmost simplicity and without introducing any but the best known properties of molecules, a great number of diverse phenomena, seemingly incapable of explanation in any other way. The only objection of the author which we can completely understand is that the theory in question—i. e., the kinetic theory of gases—seems to him incompatible with his own favorite doctrine that molecules are inelastic. Should he have any hesitation in pitting his *a priori* idea against so widely received a theory, it should relieve him to know that the supposed antagonism arises only from his own misapprehension. *No elasticity is assigned the molecules in the kinetic theory, but only an insuperable, repulsive force which causes the molecules to repel each other when they are brought sufficiently near together.* The reader who has any interest in following the author in his attempt to show that Maxwell and his colaborers were guilty of a long series of fallacies and errors in attempting to prove the theory in question, may read the chapter, as an abstract is impossible.

So "no elasticity is assigned to the molecules in the kinetic theory." Well, that is startling news indeed ! I hope it has been conveyed to Sir William Thomson, who at latest accounts was still engaged in the arduous, but, as we are now informed by Professor Newcomb, utterly useless study of vortex-rings, which he hopes to make available as substitutes for elastic atoms or ultimate molecules. At the last meeting of the British Association Sir William Thomson read a paper "On the Average Pressure due to the Impulse of Vortex-Rings on a Solid," of which an abstract is published in "Nature" for May 12, 1881 (vol. xxiv, pp. 47, 48). In this paper Sir William says :

> The pressure exerted by a gas composed of vortex-atoms is exactly the same as is given by the ordinary kinetic theory, *which regards the atoms as hard elastic particles.*

I do not deem it necessary to multiply quotations from the writings of other scientific men in support of my statement that the kinetic theory of gases can not dispense with the assumption of the elasticity of ultimate molecules. No intelligent reader who has glanced at page 42 of my book can be in any doubt as to what is taught on the subject by the founders and promoters of the theory in question. But I will add one citation, because it is from a book to which I shall have occasion to refer for another purpose. The most thorough mathematical treatise on the kinetic theory of gases, indorsed as such by Clerk Maxwell, is the well-known little book of Henry William Watson. It is in the form of propositions ; and the very first words of the first proposition are these :

A very great number of smooth, *elastic* spheres, equal in every respect, are in motion within a region of space of a given volume, and therefore occasionally impinge upon each other with various degrees of relative velocity, and in various directions.

The italics in this passage, as well as in all past and future quotations, are mine.

In justice to Professor Newcomb, however, we must look at his entire sentence, which is this: "No elasticity is assigned to the molecules in the kinetic theory, *but only an insuperable, repulsive force, which causes the molecules to repel each other when they are brought sufficiently near together.*" This information, Professor Newcomb hopes, will "relieve me." I am indeed relieved! What the learned professor tells me in the last part of his sentence certainly simplifies matters to the last degree. All that needs be assigned to the molecules is an "insuperable repulsive force." Such a force is the greatest convenience for the physicist that can possibly be devised; it not only effects a simple and satisfactory solution of the difficulties set forth in my fourth and eighth chapters, but it enables us at once to get over every other difficulty that may be suggested. It is singular that Sir Isaac Newton did not understand this when he was distressed about the mechanism of gravitation; for, obviously, all that is required to explain it is to assign to the molecules an *attractive force.* Sir Isaac's ignorance is all the more remarkable because, coming to think of it, I now recollect that the philosophy of which Professor Newcomb is the able exponent was very clearly set forth, just fourteen years before the appearance of Newton's "Principia," in a profound metaphysical treatise published by one Jean-Baptiste Poquelin (otherwise called Molière) under the somewhat whimsical title "Le Malade Imaginaire." Toward the close of that great work (which is in the form of dialogues), one of the interlocutors, Bachelierus, philosophizes as follows:

"Mihi a docto doctore
Domandatur causam et rationem quare
Opium facit dormire.
A quoi respondeo
Quia est in eo
Virtus dormitiva
Cujus est natura
Sensus assoupire."

Of course, we are not to be embarrassed by anything John Bernoulli has written about "insuperable forces" as mathematical or physical functions; nor is it worth while to be disturbed by considerations respecting the effect of their assumption upon the doctrine of the conservation of energy.

Professor Newcomb's indignation at my treatment of the kinetic theory of gases is very great indeed. "There is no theory of modern physics," he says, "the processes supposed by which are invisible to

direct vision, which is more thoroughly established than this. *It explains with the utmost simplicity, and without introducing any but the best-known properties of molecules, a great number of diverse phenomena seemingly incapable of explanation in any other way.*" Now, it is a great pity that these glad tidings did not reach Professor Clerk Maxwell before he was laid to rest in his early grave. They would certainly have been a great comfort to him, and possibly might have prolonged his life. For there is reason to suspect that in his latter days he arrived at conclusions respecting the kinetic theory of gases which bear a strange resemblance to my own. Being, not a scientific dogmatist, but an honest and candid investigator in search of truth, he came to see with ever-increasing clearness that the difficulties of his favorite theory beset not only its fundamental assumptions, but also their inevitable consequences, especially in their bearings upon the theory of heat. After the appearance of Watson's treatise already adverted to, on the 26th day of July, 1877, he published in "Nature" (vol. xvi, No. 404) a review of it, in which he considered the significance of Mr. Watson's propositions in connection with certain matters discussed on pages 97, 99, and 127 of my book. And thereupon he made this declaration ("Nature," vol. xvi, p. 245) :

> The clear way in which Mr. Watson has demonstrated these propositions leaves us no escape from the terrible generality of his results. Some of these, no doubt, are very satisfactory to us in our present state of opinion about the constitution of bodies, but there are others which are likely to startle us out of our complacency, *and perhaps ultimately to drive us out of all the hypotheses in which hitherto we have found refuge into that state of conscious ignorance which is the prelude to every real advance in knowledge.*

I hope, by-the-way, that this last remark of the great scientist will be pondered by those who complain that, after demolishing, as they imagine, all current physical theories, I leave them in the midst of ruins, and do not at once present them with a golden key for unlocking all the mysteries of the universe, or, like Puck, in "Midsummer-Night's Dream," "put a [theoretical] girdle round about the earth in forty minutes."

Before I leave this subject, I take the liberty of quoting another passage from the same article, which Professor Newcomb, if he knows *anything* about the discussions to which the kinetic theory of gases has given rise, will find instructive. Speaking of Boltzmann's attempt to reconcile the elasticity of atoms with their rigidity by increasing their co-efficients of elasticity *ad infinitum*, so as to make them practically rigid—a supposition also developed in an essay of Hugo Fritsch in Königsberg, entitled "Stoss zweier Massen unter der Voraussetzung ihrer Undurchdringlichkeit behandelt," which does not seem to have fallen under Professor Maxwell's notice (and, I may add, a supposition of which Professor Newcomb's "insuperable force" may

be a vague reminiscence)—Maxwell says ("Nature," vol. xvi, pp. 245, 246):

But, before we accept this somewhat promising hypothesis, let us try to construct a rigid-elastic body. *It will not do to increase the co-efficients of elasticity without limit till the body becomes practically rigid.* For such a body, though apparently rigid, is in reality capable of internal vibrations, and these of an infinite variety of types, so that the body has an infinite number of degrees of freedom.

The same objection applies to all atoms constructed of continuous, non-rigid matter, such as the vortex-atoms of Thomson. Such atoms would soon convert all their energy of agitation into internal energy, and the specific heat of a substance composed of them would be infinite.

A truly rigid-elastic body is one whose encounters with similar bodies take place as if both were elastic, but which is not capable of being set into a state of internal vibration. We must take a perfectly rigid body and endow it with the power of repelling all other bodies, but only when they come within a very short distance from its surface, but then so strongly that under no circumstances whatever can any body come into actual contact with it.

This appears to be the only constitution we can imagine for a rigid-elastic body. And, now that we have got it, the best thing we can do is to get rid of the rigid nucleus altogether, and substitute for it an atom of Boscovich—a mathematical point endowed with mass and with powers of acting at a distance on other atoms.

But Boltzmann's molecules are not absolutely rigid. He admits that they vibrate after collisions, and that their vibrations are of several different types, as the spectroscope tells us. But still he tries to make us believe that these vibrations are of small importance as regards the principal part of the motion of the molecules. He compares them to billiard-balls, which, when they strike each other, vibrate for a short time, but soon give up the energy of their vibration to the air, which carries far and wide the sound of the click of the balls.

In like manner, the light emitted by the molecules shows that their internal vibrations after each collision are quickly given up to the luminiferous ether. If we were to suppose that at ordinary temperatures the collisions are not severe enough to produce any internal vibrations, and that these occur only at temperatures like that of the electric spark, at which we can not make measurements of specific heat, we might, perhaps, reconcile the spectroscopic results with what we know about specific heat.

But the fixed position of the bright lines of a gas shows that the vibrations are isochronous, and therefore that the forces which they call into play vary directly as the relative displacements, and, if this be the character of the forces, all impacts, however slight, will produce vibrations. Besides this, even at ordinary temperatures, in certain gases, such as iodine gas and nitrous acid, absorption bands exist, which indicate that the molecules are set into internal vibration by the incident light. The molecules, therefore, are capable, as Boltzmann points out, of exchanging energy with the ether. But we can not force the ether into the service of our theory so as to take from the molecules their energy of internal vibration, and give it back to them as energy of translation. It can not in any way interfere with the ratio between these two kinds of energy which Boltzmann himself has established. All it can do is to take up its own due proportion of energy according to the number of its degrees of freedom. We leave it

to the authors of "The Unseen Universe" to follow out the consequences of this statement.

I may safely take it for granted after this, I presume, that, while Professor Newcomb may have a vocation for expounding and defending the kinetic theory of gases, he has no special call, as he supposes, to stand up for Clerk Maxwell and his opinions.

It is hardly necessary to say that Professor Newcomb does not honor my objections to the kinetic theory of gases with any notice or attempt at refutation. He observes that "an abstract of them is impossible," which is to be regretted, for, if he had undertaken to give us one, we should undoubtedly have learned some noteworthy things. The task of making such an abstract does not appear to be very difficult. What I insist on is, that every valid physical theory is essentially a simplification and not a complication, a reduction of the number of unrelated facts which it undertakes to account for, and not a mere substitution of many arbitrary assumptions of unknown and unverifiable facts for a few known facts—that is to say, speaking in the language of mathematics, that every true physical theory is in effect a reduction of the number of independent variables representing the phenomena to be explained. And I show that the kinetic theory of gases not only fails to satisfy this requirement, but is a complete reversal of a legitimate scientific procedure. This is the sense of the passage which Professor Newcomb parades before the unwary reader, whom he ought to have shocked still more with my horrible suggestion (which I now deliberately repeat) that a gas is in its nature a simpler thing than a solid, and that no attempt to account for its properties by taking those of a solid as a basis and making arbitrary additions to them is likely ever to succeed.

It is not a little instructive to note the character of sacredness ascribed by persons of Professor Newcomb's frame of mind to dominant physical theories, and the violence with which they repel every attempt to point out their defects. My reviewer in "The Critic" is almost beside himself after reading my "assault" on "that magnificent fabric of science, the undulatory theory of light and heat." Before he pelts me again with his missiles, he will do well to look and see who is standing at the place to which he directs them. There is at Harvard University a most learned and laborious scientist whose merits as an original investigator are at least equal, if not superior, to his inestimable services as an expounder of scientific truth, and the extent of whose attainments is no less conspicuous in his memoirs and books than the clearness of his intellect—Professor Josiah P. Cooke, Jr. In May, 1878, Professor Cooke published a lecture on the radiometer in this journal ("Popular Science Monthly"), in which he had occasion to speak of the undulatory theory of light and the luminiferous ether. And there (pages 11, 12) we find this language :

But turn now to the astronomers, and learn what they have to tell us in re-

gard to the assumed luminiferous ether through which all this energy is supposed to be transmitted. Our planet is rushing in its orbit around the sun at an average rate of over 1,000 miles a minute, and makes its annual journey of some 550,000,000 miles in 365 days, 6 hours, 9 seconds, and $\frac{6}{10}$ of a second. Mark the tenths; for astronomical observations are so accurate that, if the length of the year varied permanently by the tenth of a second, we should know it; and you can readily understand that, if there were a medium in space which offered as much resistance to the motion of the earth as would gossamer threads to a race-horse, the planet could never come up to time, year after year, to the tenth of a second.

How, then, can we save our theory, by which we set so much, and rightly, because it has helped us so effectively in studying Nature? If we may be allowed such an extravagant solecism, let us suppose that the engineer of our previous illustration was the hero of a fairy-tale. He has built a mill, set a steam-engine in the basement, arranged his spindles above, and is connecting the pulleys by the usual belts, when some stern necessity requires him to transmit all the energy with cobwebs. Of course, a good fairy comes to his aid, and what does she do? Simply makes the cobwebs indefinitely strong. So the physicists, not to be outdone by any fairies, make their ether indefinitely elastic, and their theory lands them just here, with a medium filling all space, thousands of times more elastic than steel, and thousands on thousands of times less dense than hydrogen gas. There must be a fallacy somewhere, *and I strongly suspect it is to be found in our ordinary materialistic notions of causation*, which involve the old metaphysical dogma, "*nulla actio in distans*," *and which in our day have culminated in the famous apothegm of the German materialist*, "*Kein Phosphor, kein Gedanke.*"

If my reviewer will compare this passage with what I have said on the undulatory theory, he will, perhaps, discover that my observations are at least proof against the charge of frivolity and irrelevancy. And it is not necessary to add, I hope, that it is no more my intention than that of Professor Cooke to call upon the physicist to throw away the undulatory theory as a working hypothesis before he has a better one.

I now come to Professor Newcomb's reflections on my discussion of transcendental geometry. Here are some of them:

In considering the author's work in detail, we begin with the subject of transcendental geometry, or hyper-geometry, as it is sometimes called. We do this because his criticisms are so readily disposed of. He speaks of the "new geometrical faith"; of the "dispute" between the "disciples" of the transcendental or pangeometrical school and the "adherents" of the old geometrical faith; of the "champions" of the old geometrical creed; of the "doctrine" of hyperspace. To the refutation of these supposed erroneous doctrines he devotes no less than sixty-two pages. Now, all his criticism is founded on an utter misapprehension of the scope and meaning of what he is criticising. We make bold to say that no mathematician has ever pretended to have the slightest evidence that space has four dimensions, or was in any way different from what is taught in our familiar system of geometry. He has not been an adherent or champion, or held any doctrine on the subject. Now and then it is barely possible that a physicist might be found—Zöllner, for instance—suggesting such a thing in a moment of aberration. But the great mass of men in their senses remain unaffected by any such idea.

Again :

Whatever we may say of the utility of such investigations, one thing is certain—they are perfectly harmless. At the very worst they can do no more injury to scientific conceptions than the careless author of an elementary algebra will do his pupil by loading an hypothetical baker's wagon with more loaves of bread than the baker could get into it. If Judge Stallo had taken up a book on algebra, found a problem the answer to which required five thousand loaves of bread to be carried by a single baker, and had devoted sixty-two pages to an elaborate statistical and mechanical proof that no wagon could possibly hold that number of loaves, his criticisms would have been as valuable and pertinent as those which he devotes to his imaginary school of pangeometry.

After reading these passages I am sorely perplexed. When Professor Newcomb penned them he had before him my extracts (in a note to page 211 of my book) from the Exeter address of Professor Sylvester, embodying a reference to the speculations of Professor Clifford, and another independent citation from Clifford's writings on page 213. And, being himself a writer on geometry of more than three dimensions, he can hardly have been ignorant of the many other pangeometrical speculations respecting the necessity of assuming the existence of a fourth dimension for the purpose of explaining certain optic and magnetic phenomena. There are mathematicians and physicists in Europe—excellent mathematicians and physicists, too—who maintain that space must have at least four dimensions, because without it a reconciliation of Avogadro's law with the first proposition of the atomo-mechanical theory is impossible. According to them, experience shows that matter has not only *extension* but also *intension*, which directly evidences the actual existence of a fourth dimension in space. Among those who advocate views like this is Professor Ernst Mach, in Prague. How, in the face of all this, Professor Newcomb could have the hardihood to assure his readers that no mathematician has ever pretended that space has more than three dimensions, I am at a loss to understand.

But it is not worth while to quarrel with him on this head ; *for his statement, that I devote sixty-two pages to the attempt at proving that space has in fact but three dimensions, is a pitiful misrepresentation, akin to the statement that I am the defender of the propositions of the atomo-mechanical theory.* In my two chapters on transcendental geometry there is not a page, not even a line, devoted to such an undertaking. I discuss two main questions : first, whether or not it is true, as Lobatschewsky, Riemann, and Helmholtz assert, that space is a real thing, an object of direct sensation whose "properties," such as the number of its dimensions and the form or degree of its inherent curvature, are to be ascertained by observation and experiment—by telescopic observation, for instance ; and, secondly, whether or not the empirical possibility and character of several kinds of space can be deduced *a priori* from the concept of an n-fold extended multiple,

or from the abstract concept "quantity," using this term as comprehending both algebraic "quantities" and geometrical magnitudes. As subsidiary to these questions I also discuss certain minor questions, such as that of the representability of non-homaloidal forms of space; but upon the proof that there is actually no such thing as non-homaloidal or four-dimensional space I do not waste a syllable. In other words (which Professor Newcomb may find more intelligible, perhaps): my first inquiry is, not whether any one has ever discovered a fourth dimension or an inherent spatial crook by looking through a telescope, but whether there would be any use or sense in trying to make such a discovery by looking through a telescope, even if we could get a base-line large enough to meet the requirements of Professor Helmholtz; and my second inquiry is, whether or not there is any world-producing potency in an algebraic formula or an "abstract noun."

Professor Newcomb claims that investigations respecting geometry of more than three dimensions are at least harmless, and even useful, inasmuch as "they have thrown a flood of light on the origin and meaning of geometrical axioms." My answer to this is, that speculations of this sort are harmless only so long as it is not pretended that they can teach us anything respecting either empirical reality or empirical possibility. And they can throw light on the origin and meaning of geometrical axioms only by giving us an insight into the nature of the forms or modes in which the world of objective reality is or may be reproduced in the intellect. But what shall we say, then, about the grin at speculation in science which stares at us from the very title of Professor Newcomb's article? If he may throw a flood of light on the foundations of geometry, by speculating about space of four dimensions, am I to be jeered at when I endeavor to direct a feeble ray from the general theory of cognition on the same subject—when I try to do methodically what he is doing at random, and without the least suspicion that anything more is necessary for the accomplishment of his purpose than skill in the handling of an analytical formula? It may be that my undertaking has not been very successful; but *in magnis voluisse sat est.* And this leads me to say a few words in answer to the intimation of Professor Newcomb and the direct charge of my reviewer in "The Critic," that inquiries into the forms and laws of thought are sheer impertinence, and of no consequence to the physicist.

In the introductory part of his article Professor Newcomb flings at me the case of De Morgan's paradoxer Smith, who fancied that he could prove the ratio of the circumference of a circle to its diameter to be exactly $3\frac{1}{8}$, by getting somebody to admit that the ratio of the circumference to the diameter is the same for all circles, and then telling him to draw *one* circle with the diameter 1 and circumference $3\frac{1}{8}$. Now, the intellectual plight of this paradoxer, who, besides assuming the very thing to be proved, failed to see that his argument would

serve equally well to establish any other ratio, and who never thought of asking himself the question whether or not a diameter 1 and a circumference $3\frac{1}{8}$ were compatible—whether or not his postulates were consistent with each other—is closely analogous to the mental predicament of certain scientific specialists who are constantly multiplying forces, superable and insuperable, and all manner of entities, with impossible or contradictory properties, for the purpose of explaining natural phenomena. When this is done with a proper insight into the nature and use of such fictions—with the understanding that they are mere devices for fixing ideas or colligating facts (to use Whewell's expression)—it is well enough. But, in many cases, the specialists have no such insight. They begin to treat the fictions here spoken of as undoubted realities, whose existence no one can question without subjecting himself to a Newcombian fustigation. Take the case of the ether, the hypothetical substratum of luminar undulations. It is first mentioned simply as a fluid of the greatest tenuity, as wholly inappreciable to the senses, and as offering no resistance to atoms or celestial spheres. Thereupon, to meet the exigencies of the undulatory theory, it is endowed with a co-efficient of elasticity thousands of times greater than that of steel. Next, at the demand of some physicist or chemist, who wants to incase his atoms or molecules in ethereal atmospheres or envelopes, it is made as soft and mobile as hydrogen gas. First, it is looked upon as continuous; then, to explain the dispersion of light, it is made discontinuous, and "finite intervals" are interposed between its atoms. But now comes Clerk Maxwell, and shows that, if the constitution of the ether were atomic, consequences would ensue upsetting the whole theory of heat; or Helmholtz and Sir William Thomson, in order to be able to construct their vortex-atoms, require it to be absolutely frictionless and incompressible, and therefore continuous; and, accordingly, it is restored again to its ancient continuity, no matter what may become of Cauchy's theory of chromatic dispersion or Fresnel's theory of polarization. Originally there is but one ether; but presently Professor Norton contends that the luminiferous ether is not available for the purpose of explaining the phenomena of electricity and magnetism. He demands a second ether, filling the same space with the first; and his demand is complied with. In a short time Mr. Hudson appears with the claim that even the phenomena of light can not be accounted for on the supposition of a single light-bearing ether; and he must have *two* luminiferous media, "each possessed of equal and enormous self-repulsion or elasticity, and both existing in equal quantities throughout space, whose vibrations take place in perpendicular planes; the two media being mutually indifferent, neither attracting nor repelling"—and, again, his request is granted without further ceremony. To cap the climax, finally arrives the pangeometer, and insists that back of and behind all these ethers there is an independently real thing, an object of direct sensation,

space, which is probably flat, but which possibly may turn out to be inherently crooked. And now, when somebody shakes his head and proposes to examine whether there is not something wrong with this whole mode of philosophizing, which mistakes crutches for limbs, and scaffolds for buildings, Professor Newcomb hurls a wooden thunderbolt at him, or a reviewer in the New York "Critic" reminds him that "the sound thinker gives himself little uneasiness respecting the laws of thought."

Now let us look for a moment at the atom. The physicist or chemist gets it originally as an ancient heir-loom, handed down from the times of Democritus or Lucretius. It is a solid body, with attachments of hooks and loops. The modern scientist takes off the attachments, and holds on to the main solid body, polishing it for his use. So this body becomes round ; but in course of time appear the mineralogist and chemist with their morphological laws, such as the law of Mitscherlich, with theories of polarity or valency, or what not ; and to accommodate them it is proclaimed that the atom is a cube or a rhomb or an octahedron, or whatever else will silence the most clamor. After a while, Kroenig or Clausius declares that, in the interest of his kinetic theory of gases, he must insist on the perfect sphericity of the atoms or ultimate molecules ; and thenceforward (for a month at least) they are spherical. But, at the expiration of the month, Maxwell points to certain anomalous facts which are supposed to be inconsistent with atomic sphericity, and he suggests that it be modified so as to give the atoms the form of oblate or prolate spheroids ; and, of course, his suggestion is adopted. In a short time some physicist rushes out of his laboratory or study, and announces that he has just obtained experimental results or arrived at theoretical conclusions requiring an utter rejection, not only of the definite figure of the atom, but of its entire bulk ; and forthwith it is subtilized into a mere center of force. But now the physicist is reminded that force must have a substratum, and that its indispensable correlate is inertia. At this juncture the pangeometer flits upon the scene, and offers the perplexed physicist his fourth dimension in which to lodge both the extension and "intension"—i. e., mass—of the centers of force, assuring him that he may have the mere punctuality of the atom in ordinary space, and behind it, in space of four dimensions, any amount of bulk and weight. At this stage of the proceedings the physicist begins to look desperate ; perhaps he is silently meditating the question, What is to become of experimental research if the properties of things can vanish *ad libitum*, and retire into the recesses of the pangeometrical regions ? And yet, woe to him who ventures to suggest to the chemist that the origin of the trouble is not in his retorts, but in the sincipital alembic through which all his results are at last distilled, or to show the physicist that there is no defect in the lenses of his microscope, but great want of achromatism in those of his intellect ! He speedily learns that the

stupid arrogance of dogmatism, which it is the special function of science to repress, has some of its most vulgar representatives in the ranks of those who claim to be, not only votaries of science, but its chosen protagonists and defenders.

Some years ago Fechner, in the first edition of his "Atomenlehre," printed an answer he had made to some one who objected to the theories of the physicists about atoms, ethers, forces, and so on. It was something like this: "I have a handful of coins. You are not pleased with the effigy and inscription, and advise me to throw them away; yet you offer me nothing to replace them but an empty purse." If that speech had been made to me, I should have met it with this reply: "The mischief is that your coins are spurious; they are base metal. Nevertheless, they may serve a good purpose as mere counters or tokens, provided you never lose sight of the fact that they are nothing more. But experience teaches that you *do* constantly lose sight of that fact, and in a short time insist dogmatically that the coins are of unquestionable intrinsic value. And, having found out that you can manufacture any amount of them at little expense, you do what all inflationists and debasers of the currency are in the habit of doing: you flood the market with stuff which must inevitably bring ruin upon the very man whom you have ensnared into the belief that he can never have enough of it, viz., the laborer who is employed in the hard work of producing the material out of which science is to be constructed. So, if you are unable to procure genuine theoretical specie to represent the scientific wealth you are intent on accumulating, and at the same time are unwilling to restrain your propensities for manufacturing spurious coin and palming it off on yourself and others as sterling cash, you had better carry your facts about in baskets or bags, and resort to the ancient clumsy method of barter."

I will not weary the reader by drawing upon the rich store-house of theoretical chemistry for further illustration of the manner in which provisional and tentative hypotheses are paraded as absolute finalities, and results of experimental research are obscured instead of being irradiated by theoretical conceits. I will content myself with a single further reference to a very recent and very remarkable exemplification of the proneness of the very ablest men of science to multiply entities and confound modes of physical interaction or forms of intellectual apprehension with indestructible things.

In the scientific journal, "Nature," for May 26, 1881 (vol. xxiv, p. 78), there is a communication from Professor Silvanus P. Thompson, containing an extract from the preface to his then forthcoming book "Elementary Lessons in Electricity and Magnetism," in which he says:

> The theory of electricity adopted throughout is, that electricity, whatever its nature, is one, not two; that electricity, whatever it may prove to be, is not matter and is not energy; that it resembles both matter and energy in one respect, however, in that it can neither be created nor destroyed.

Accordingly, Professor Thompson supplements the doctrines of the "Conservation of Matter" and "Conservation of Energy" with the new doctrine of the "Conservation of Electricity," which, indeed, is the title prefixed to his communication.

There are, of course, thoughtful physicists (and their number is increasing from day to day) who do not share the delusion that every momentary device for sorting and grouping facts is to be hailed as a new scientific revelation, and who do not dream of calling upon any one to uncover his head before every passing conceit as though it were an eternal truth. But, unfortunately, these men are not always in the high places, and are averse to obtruding themselves in public as vindicators of the authority of science.

I certainly cherish sentiments of the sincerest admiration and respect for the high-minded and generally modest men who devote their energies to the extension of the bounds of knowledge, and, in the interest of thorough and effective work, shut themselves up in narrow and dingy workshops from whose windows a wide survey of the scientific horizon is difficult or impossible. And I appreciate fully the impropriety of troubling and interrupting them with idle and frivolous criticisms and suggestions. I know that they are under the necessity of arranging and combining their crude materials upon such principles and hypotheses as they have at hand—that they can not make bricks without straw. But when a scientific specialist appears as an intruder in discussions for participation in which his habitual occupations have tended, not to qualify, but to disqualify him; and when, instead of listening and saying what he has to say respectfully, he turns to the crowd and vociferates about "charlatans," "pretenders," and "paradoxers," my thoughts involuntarily run into the words of an old Greek which have been stored in my memory since my boyhood days:

Ὅς δέ κε μητ' αὐτὸς νοέῃ μήτ' ἄλλου ἀκούων
Ἐν θυμῷ βάλληται, δδ' αὖτ' ἀχρήϊος ἀνήρ.